regularity, and constituting along each gill a kind of grating bearing a close resemblance to the teeth of a comb. The laminæ of which this grating is composed become gradually narrower from their fixed to their free extremities; they are of a dark olive colour, of a hard texture, and highly elastic, but at the same time brittle, and easily snapping off when urged beyond a certain point.

"The office which Mr. Allman assigned to these branchial appendages was that of strainers, by which the water before coming in contact with the branchiæ is freed from extraneous bodies, which would otherwise interfere with the functions of respiration. The objection which might be urged to this view, namely, that the other sharks are without any such arrangement appeared to him of no weight, as we know but little of the habits of the Basking Shark, and as those which we do know would lead us to believe that the structure just described is admirably adapted to the fish's peculiar mode of life. The Basking Shark must be entirely free from the voracious disposition so characteristic of the allied genera. Its teeth are little more than tubercles, and quite unfit it for the life of carnage led by other sharks. Its food must accordingly be found among the less resisting inhabitants of the ocean; and as the Basking Shark will therefore be driven to feed near the bottom and among seaweeds the existence of the branchial appendages will admit of an easy explanation. We must thus at once perceive the admirable adaptation of this interesting arrangement to the habits of an animal which would otherwise be subjected to the constant annoyance of having its branchiæ clogged with the floating fronds of sea-weeds, a circumstance which the anatomical structure alone would otherwise render more liable to occur in this than in the other sharks, as the openings to the branchiæ in the *Selachus maximus* are of enormous size, and the branchiostegous membranes particularly loose." GEO. J. ALLMAN

The Birds of Kerguelen Island

MY attention has been called to a review of Dr. Kidder's "Report on the Ornithology of Kerguelen Island," in NATURE of the 10th instant (p. 317, *supra*). Will you kindly permit me to express regret that the reviewer should have alluded to priority of publication of the results of the American and English expeditions to that island? To many persons his remarks on this point will appear to be ungenerous and needlessly sarcastic to the foreign naturalists. The subject is a delicate one, and I am sorry to have occasion to mention it, especially as an Englishman should be the last to approach it.

The reviewer will doubtless admit that when three naturalists are simultaneously sent to work independently of one another in the same neighbourhood, it is almost inevitable that one will anticipate the work of the others, and yet that there is nothing to boast of if he does. In the present instance, being bound to regard the interests of my employers in my collection, I hastened the issue of preliminary diagnoses of the novelties contained in it, to secure their types from alienation to foreign museums. The result of this was the acquisition by the English of the types of all the new species in my collection excepting those of one bird (which has recently been described as new by the Germans), and those of two Annelids, and three lichens, and perhaps a moss pre-occupied by the Americans. We could well have afforded to lose nine or ten times as many, and should still have retained a fair proportion of the whole number for English museums. The reviewer, therefore, might have done well if he had censured the rapacity of the English in grasping the lion's share of the type-specimens; but it was rather too bad of him to attribute to my fellow-workers small feelings of jealousy with reference to the Americans being the first in the field with their final reports, of which they are not conscious. The Americans have kept us fully informed as to the progress of their reports during the period of their preparation, by letter and by the transmission of advance sheets; and the English final reports will no doubt be ready at the time appointed by the Royal Society. If the Germans publish their results in the meanwhile, we shall have the advantage of including references to their work among our citations.

The reviewer is perhaps unaware of the publication of another Bulletin by the Americans, containing, amongst other information relating to Kerguelen Island, further ornithological particulars. It was issued more than a month ago.

A. E. EATON
Naturalist accompanying the English
Transit of Venus Expedition to
Kerguelen Island in 1874

Aug. 24

Antedated Books

I AM ready to give the Editor of the Zoological Society's *Transactions* credit for desiring to set a good and not a bad example; but, since a man seldom thinks that which he does to be wrong, the simple assertion of his opinion that it is the former and not the latter is not enough. Whether the papers in those *Transactions* are antedated by one month (as he admits) or by several months is merely a matter of detail. The practice of antedating is equally faulty in principle. If their editor would add the correct date of publication on the covers of the several parts, as is done with the *Proceedings* of the Royal and the *Journal* of the Linnean Society, he might give whatever date he pleases anywhere else as that of his latest revision.

ANOTHER F.Z.S.

Earthquake in Nithsdale, Scotland

ON the morning of the 12th current, at 3 o'clock, Mr. Robson, of the schoolhouse of Penpont, Dumfriesshire, was awakened by a sharp shock of earthquake and heard its detonations. On inquiry the same shock had been felt at the schoolhouse of Tynron, by Mr. Laurie; and over an area of several parishes around the upper course of the Nith the shock was felt, causing walls to vibrate and cupboard dishes to tingle. Two concussions of less violence were felt between 11 and 12 o'clock on the previous evening. The morning papers of the 14th report that a severe shock of earthquake had been felt at Athens on the morning of the 12th. It would be interesting to know the exact time when the shock was felt in Greece. On April 16th, 1873, at 9.55 P.M., a similar shock to that experienced last week was felt in the same districts of Nithsdale. I recollect communicating a short notice of it to NATURE at the time, as I had heard the strange sound, but on this occasion I did not hear it.

Tynron Schoolhouse, Aug. 23 JAMES SHAW

P.S.—Since writing the above I have received confirmation of the event from several other reliable witnesses. It seems to have been most plainly felt in the parishes of Morton, Penpont, Keir, Tynron, and Glencairn, to the west of the Nith. J. S.

The Cuckoo

THE usual manner in which the cuckoo in June "alters his tune," is by doubling his first syllable, and the "cuc-cuckoo, cuc-cuckoo" is then usually, if not always, followed by the single "cuc." This is certainly the case both near London and in the Midlands. E. H.

ABSTRACT REPORT TO "NATURE" ON EXPERIMENTATION ON ANIMALS FOR THE ADVANCE OF PRACTICAL MEDICINE[1]

VII.

THERE occur to me a few other illustrative series of researches, in which scientific and practical medicine have been advanced by experimentation on the lower animals. Some of these I will state in terms as brief as possible in the present paper.

Experimentation in respect to the Disease called Cataract.

Dr. Weir Mitchell, of Philadelphia, in the year 1869, made the original and remarkable observation that if a part of the body of a frog be immersed in simple syrup, there soon occurs in the crystalline lens of the eyeball an opaque appearance resembling the disease called cataract. He extended his observations to the effects of grape sugar, and obtained the same results. He found that he could induce the cataractic condition invariably by this experiment, or by injecting a solution of sugar with a fine needle, subcutaneously, into the dorsal sac of the frog. The discovery was one of singular importance in the history of medical science, and explained immediately a number of obscure phenomena. The co-existence of the two diseases, diabetes and cataract, in man, had been observed by France, Cohen, Hasner, Mackenzie, Duncan, von Graafe, and others, and von Graafe had stated that after examining a large number of diabetic patients in different hospitals, he had found one-fourth affected with cataract. Before Mitchell's observation there was not a suspicion as to the reason of this connection, and a flood of light, therefore, broke on the subject the moment he proclaimed the new physiological fact. Still more Mitchell showed that the cataract he was able to induce by experiment was curable also by experiment, a truth which will one day lead to the cure of cataract without operation. Then, but not till then, the splendid character of this original investigation, and the debt that is due to one of the most original, honest, laborious workers that ever in any age cultivated the science and art of medicine, will be duly recognised.

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[1] Continued from p. 341.

When the news of Mitchell's discovery reached us here, I took up the investigation at the point where he had left it. The fact he had announced was found to be indisputable. From a patient in one of our large hospitals, who was suffering from diabetes and double cataract, a specimen of the sugar excreted was obtained, and from that specimen the cataractous disease was induced in the frog, and afterwards removed. The experiment was conducted with the animal kept in an anæsthetic atmosphere, and was found to answer just as well as in the ordinary atmosphere; in fact, the experiment succeeded best with frogs when it was rendered free of all pain, as spasmodic movements, which may occur if the process of production of cataract is rapid, and which may suddenly kill, are prevented. Since the introduction of chloral hydrate, that anæsthetic has become a still more useful agent in this research, since its own action runs in line with the experiment, and the anæsthetic can be introduced in actual combination with the substance producing the cataract.

In warm-blooded animals I learned that the cataractous change could be brought about immediately after death. Several of the experiments were made therefore on the head of the sheep after the animal had been killed at the slaughter-house in the ordinary way, the fluid being injected through an artery. In other warm bloods the death was first induced by one of the anæsthetic vapours, and the fluid used was either injected into the peritoneal cavity or through the aorta.

The line of research which I carried on in continuation of Dr. Mitchell's discovery was for the purpose of determining the cause of the cataractous change and the influence of other agents in producing it. It occurred to me that the change was possibly due to the influence of saline matter on the pure colloidal lens, and if this were true the cataract ought to be induced by other substances than sugar. Any of the soluble crystalloids might produce it, and as there are many of these in the blood, there might be other cataracts than such as are produced by sugar in the diabetic subject. The research was therefore pursued with all the soluble salts belonging to the blood, and with the result of producing cataractous change with them all. In the end it was deduced that whenever the specific gravity of the blood is raised, by the presence of saline matter in it, to 10 degrees above the normal standard, and is sustained in that state for a short time, cataract is the result, and is maintained so long as the blood continues of the same specific weight. It was also found that the cataractous condition caused by the soluble blood salts was removable on the elimination of the added saline and the reduction of the blood to its natural equilibrium. At the same time there was observed to be a difference in the characters of the cataracts produced. Some of the saline cataracts were harder than the sugar cataracts and less easily curable. Those salts which are most fixed in their chemical constitution and at the same time are most soluble, produce the hardest cataracts. Those salts which are most easily decomposed, such as urea, are least effective in inducing the pathological change.

The change was found to commence, as a rule, in the posterior part of the lens, and after beginning as an imperfectly defined hazy spot it extended gradually through the whole structure, causing a pearly whiteness and complete opacity. In the process of cleaning of the lens the posterior part was the last to become transparent, but without exception the whole structure of the lens regained its crystalline clearness and its perfect function when the specific weight of the blood was reduced to its natural standard, if the circulation of fluid through the lens continued.

In these experiments two illustrious scholars, now lost to science, took the warmest interest, the late Professor Graham and the late Sir David Brewster. Both lent to me their valued observation. Graham saw in the experimental facts the first application in physiological pathology of his great discovery of the mutual action of colloidal and crystalloidal substances. Sir David drew some most ingenious inferences as to the physical cause of the opacity, tracing it to a process of crenation on the margins of the fibres of the lens. The greatest interest was naturally excited throughout the medical profession. In this production of cataract the first visible demonstration was offered of the synthesis of a well-known disease. It is now certain that if the specific gravity of the blood be raised rapidly a few degrees by a crystalloidal substance, cataract is the direct result. Recently Dr. Sansom saw this event in the case of a young woman suffering from diabetes, who became, in a few days, stone blind from cataract in both eyes; and, indeed, the cause of diabetic cataract is now made quite plain. But the end of the discovery is not reached with this fact, important though it be. The mode of production, in man and the lower animals, of the slowly advancing cataract, from which so many persons are rendered permanently blind, is after the same process, with a different saline, acting in a slower degree; and the inference is fair that some particular forms of diet are conducive to the disease. When the whole series of facts which Mitchell commenced to unfold are completed, the disease cataract will be understood in full. Its physical pathology is already understood, and if the operative art of the surgeon be not quenched by another mode of cure resulting from his discovery, it will be by the better art of prevention of the disease.

Experimentation on Pectous Changes.

The observations on cataract above described led me to follow out other lines of inquiry in respect to the action of saline substances on living and dead colloidal matter. I thus found that when a saline solution of a colloid, such as albumen, is brought into contact with a living colloidal structure like the peritoneum, the saline solvent is rapidly removed into the circulation, and the colloidal plastic substance is left on the true membrane as a false membrane, by which contiguous membranes are agglutinated together. I found further that if the blood or serous part of the blood in a fluid saline condition exudes into a serous cavity, the same simple physical process goes on, the saline and watery parts, including the colouring matter of the blood, passes back into the circulation through the membrane and the colloidal fibrine and albumen are left in form of false membrane or of band, on the true membrane. The experiment illustrates how inflammatory exudations, as they are called, are produced, and how adhesions and adhesive constrictions are formed after inflammatory serous diseases. The experiments on animals by which these results were arrived at, were all conducted under anæsthesia, and were perfectly painless.

In another analogous series of inquiries conducted in the same manner, I found that if the blood were surcharged with urea, a portion of albumen would pass out of the body by the urinary secretion without the institution of any marked morbid change in the structure of the kidney. This fact led me to ask whether albumen diluted with water and charged with urea would pass through a dead membrane, by dialysis, and I found it would. These facts have bearings of the most singular kind on the disease albuminuria. They show, amongst other things, that the presence of albumen in the renal secretion is not, of necessity, a sign of structural disease of the kidney as has been supposed, and they account for the anomalous illustrations that are met with of temporary albuminuria as a disease. The experiments explain also the cause of that coagulation of the blood which occurs during those exhaustive diseases, such as cholera, in which the saline and watery parts of the blood are drained away.

The same line of research suggested to me a new experimental reading, conducted by experiment on dead animal matter, of the cause of the pectous change called, commonly, coagulation. This change I find is always produced in fluids containing soluble crystalloidal matter, colloidal matter, and water, whenever the relationship between the colloid and the water is disturbed by modification of the crystalloid. The crystalloid forms the connecting link by which the water is held in fluid combination with the colloid. If the crystalloid be withdrawn, then the molecular attraction of the colloid for its own parts commences, and the contraction called coagulation is set up with expulsion of water from the clot by the contraction of cohesion. If, on the other hand, crystalloid be added in excess, so as to absorb an excess of water, coagulation is also set up. Or again, if water held by condensation in a saline solution of a colloid—as the fibrine is held in the living blood for example—be allowed to escape, coagulation is the result.

Connected with these studies, but carried out long before them, are some experiments I made with urea, in which, by hypodermic injection of that animal salt, in free quantities, into the body of an animal, symptoms of unconsciousness and convulsion, like the symptoms of uræmic poisoning which occur in some cases of scarlet fever, were induced. The result to practice from these researches was to discover that the symptoms were removable by the abstraction of a little blood, and the application of this practice in examples of uræmia in man, has been the means of directly saving several lives. The result to physiological science was the fact that when from any circumstance the living blood is charged with a soluble saline body much beyond what is natural, the effect is a convulsion which recurs at intervals, as if the blood surcharged with the salt were conducting some exciting current to the muscles so rapidly that the reserve store of force in the nervous centres ran down or was completely discharged at once and had to wait to be re-supplied, at the end of each discharge, before another convulsion could be excited.

Dilution of the Blood and Feeding by the Veins.

In two great epidemics of cholera which I observed, it was impossible not to see that the cause of rapid death was, in most cases, the sudden reduction of the amount of water in the body. In some instances where all consciousness appeared to have passed away and death was declared, the recurrence of movements in the limbs of the apparently dead, suggested that in the strict sense of the word there was remaining life. In some instances the effect of injecting saline solutions into the veins had such an astounding temporary effect in bringing back the consciousness, it seemed as if we had in our hands a sure remedy, if we knew how to use it, for the worst forms of the fearful malady. The practice led me to experiment on the possibility of introducing water into the body by other channels than the veins, so that it might be gradually absorbed and might re-supply what was being lost by the watery discharges from the bowels. I therefore, in 1854, injected distilled water into the cellular tissue of anæsthetised animals subcutaneously, and also into the peritoneal cavity. The difficulty of introducing any sufficient quantity into the cellular tissue prevented that method from being followed; but with the peritoneum it was different. Into the peritoneal cavity I found not only that water, at the temperature of the body, can be introduced, through a hollow needle, without any danger whatever, but that the fluid is rapidly absorbed, and may be absorbed until as much as amounts to the fifth part of the weight of the animal is introduced into the organism. The difficulty I encountered in bringing into practice this simple means of re-supplying the body with water in cholera, lay in the fear that was expressed respecting injuries to the peritoneum. The plan was nevertheless once tried in a hopeless case in the human subject in 1854, and with perfect success in promoting recovery. At the present time, with our improved instruments for injection, and better knowledge of operations on the peritoneum, the method would be certainly applied in another outbreak of acute cholera, and I believe with most successful results. Beyond the directly practical, physiology gained a point by these researches. The experiments showed that when the blood is diluted by the addition of water, beyond a fifth of the weight of the animal, *i.e.*, by the addition of a pound of water to the blood of an animal weighing five pounds, an unconscious condition of the body is induced, with sleep, with paralysis of muscle, with reduction of temperature, and with death, if the natural balance be not quickly restored. Still further by pursuing the investigation into a comparison of the specific weight of the blood, and the specific weight of a fluid excretion such as the urine, I found that in some forms of serous dropsy attended with a very low specific weight of the fluid excretions, the blood when reduced in specific weight approaching to the specific weight of the secretions, is thrown out with the utmost ease into the serous cavities by the pressure of the circulation, is not returned by the osmotic ingoing current back into the circulating channels, and so accumulates in the serous sacs, giving rise to the phenomenon of serous dropsy.

Experimentation on the Action of Alcohol.

A very large number of my researches by experimentation have had reference to the action of medicines or of chemical substances intended to be applied as foods or as medicines to the animal body. Some of these, such as chloroform, methylene-bichloride, and nitrite of amyl, have already been noticed, but they are a small number compared with all that have been physiologically investigated. By subjecting animals of different species to the action of alcohol, I made clear what had only been surmised previously, that alcohol reduces the animal temperature. I also found that, like nitrite of amyl, alcohol produces what is called its stimulant action, by paralysing the vessels of the minute circulation. By the same course of experiment I learned that the exposure of an animal to a degree of cold that is perfectly harmless when the animal is free of alcohol, is certainly fatal when the animal is narcotised from the action of alcohol. By pursuing the research so as to include in it the heavier alcohols, such as butylic alcohol and amylic alcohol (fusel oil), I learned for the first time that the more injurious effects of some of the common alcoholic drinks sold for the uses of man are due to these exceedingly poisonous compounds; and by observing the action of alcohol, when the action is long-continued, on the visceral organs, the various organic changes it specifically engenders, independently of all other coincidental causes of disease, were accurately determined. In a word, all my researches of a physiological kind on the action of alcohol, from which so much has been gathered in respect to the utter uselessness and the great harmfulness of that potent poison, have been made from observation of its effects on the inferior animals. Its effects in reducing temperature, in reducing vascular tension, in reducing muscular power, in destroying the action of the animal membranes, in impairing the structures of vital organs, could never have been certainly demonstrated if the lower animals had not formed the field of experimental investigation. In these experiments the lower animals suffered neither more nor less than millions of those human animals who indulge in alcohol, and I am sorry to say that, like the human animals, many of them became too fond of the agent that was producing their certain deterioration. I can but feel sure that a great number of facts of the most practical kind sprang from these researches on alcohol. To them also should be added one other addition to physiology. I traced out, in watching the effects of the heavier alcohol from the lighter of the series, the singular law that the physiological action of an organic chemical substance is intensified by the increase of its specific weight. Thus butylic alcohol is more pronounced in its action than methylic, chloroform than chloride of methyl, and so on through all the series of organic compounds.

Experimentation on Septine.

In 1864 the death from diphtheria of one whose life was dearer to me than my own, led me to study more carefully than I had before, the process of secondary absorption of secretions from diseased abraded surfaces. In the case in question I felt sure that the death was due to the absorption of poisonous secretion from the ulcerated throat and from the nasal passages. There was at that time no light to guide me to the truth except the experiments of Gaspard, Majendie, and Sedillot, which bore rather on the action of purulent matter and of decomposing blood upon the body, than on secretions formed during disease. I felt it right, therefore, to seek for further information by experiment, and this gave rise to the first steps of a research which has since assumed great importance. In the latter part of 1864 some fluid secretion had to be removed from the peritoneum of a patient suffering from surgical fever, on whom Mr. Spencer Wells had performed ovariotomy. The fluid, which was quite free from decomposition, was applied by inoculation to a healthy lower animal—a rabbit. It produced a special form of disease analogous to that from which the human patient was suffering. The secretions of the infected lower animal were tested in turn on another healthy animal, and were found to be equally and specifically poisonous. The same was extended through four series with like results. Some of the original fluid was next treated with the view of ascertaining if the poisonous principle in it could be isolated, and a series of salts were obtained which were found to possess a poisonous and progressive poisonous action like the original fluid. A poisonous organic base seemed, in fact, to be present in the peritoneal secretion, to which poison, whatever its nature might be, I gave the name of *septine*. I afterwards made a series of experiments to determine what agents destroy the activity of the poison, and from the whole of the inquiry, I was brought to a theory of the epidemic diseases which I have specially announced, and which will, I believe, hold its own, viz., that these diseases, are all glandular diseases, and that their poisons are specifically nothing more than the secretions of the glandular structures in a modified condition; that they may be produced with or without infection, and that they act in producing the acute symptoms of disease, after their absorption, by their effect, primarily, on the nervous system, and secondarily, on the blood. Recently, I have endeavoured to demonstrate that the fever which the septinous poisons produce is brought about by their power of liberating oxygen from the blood, and that those agents which counteract this action most effectively are the true febrifuges. As yet all such counteracting agents—one of which, quinine, is the best example—are clumsy and slow in their neutralising effect. But chemistry has agents much more potent, and the day, I am quite sure, is not far off when we shall have given to us neutralising agents for the contagious fevers which will be as refined and potent as the poisonous agents that produce the fevers, and which will cure fevers by inoculation from the lancet-point, as certainly as small-pox, or other infectious malady, is now producible by the process of inoculation of small-pox septine. By-and-by, and this will probably be the earliest step, we shall find a vapour, to inhale which will have the desired effect, and will be rapid in operation. This principle made perfect, there will be no such thing as a necessary death from an infectious disease.

At present, the view that the poisons of the spreading diseases are merely animal secretions, like the poisonous secretion of the cobra or the changed saliva of the dog under rabies (canine madness), removes all the mystery that surrounds them, and the various plans for preventing the distribution of the poisons and of infection is rendered common sense and simple to the extremest degree.

Experimentation on Painless Extinction of Animal Life

The latest experimental researches which I have conducted on lower living animals have had for their object the discovery of a ready, cheap, and innocuous method for killing without pain those animals which are destined, as yet, for the food of man. If the labour of the physiologist be allowed to progress, the day will soon arrive when the slaughter of animals for food will become unnecessary, since he will be able to so transmute the vegetable world as to produce the most perfect and delicious foods for all the purposes of life without calling upon the lower animal world to perform the intermediate chemical changes. But until this time arrives, animals will have to be slaughtered, and my research has been directed to make a process which at present is barbarous and painful, painless in the most perfect degree. For this purpose the various modes of rapid destruction of life—by powerful electrical discharges, by rapid division of the medulla oblongata, and by the inhalation of various narcotic vapours, have been carried out. The experiments, which have been exceedingly numerous, have led me to the conclusion that the most perfect of the painless methods of killing is by the inhalation of carbonic oxide gas. So rapid and complete is the action of this gas, that I may say physiological science has done her part, as far as it need be done, for making the painless killing of every animal a certain and ready accomplishment, an accomplishment also so simple that the animal going to its fate has merely to be passed through the lethal chamber, in order to be brought in senseless sleep into the hands of the slaughterer. The application of teaching and the putting into practice this humane process lies now with the world outside science; but to insure its acceptance, all the force of selfishness, of prejudice, and of practical apathy for the sufferings of the animal creation, have to be overcome. There is a great deal of talk and a great deal of sentiment abroad on the question of the sufferings of the lower animal kingdom, but when an attempt is made to relieve those sufferings by the invention of methods for operating, surgically without the infliction of pain, or for painless killing, the true and vital sympathy which one would expect in support of such practical and humane efforts, until they are made perfect and universal, can scarcely be said to be found at all. With the exception of a few, not a dozen altogether, of really humane ladies and gentlemen, I have found no one, out of the ranks of science, in the least interested in the saving of sufferings to which I am now directing attention. The man of science stands and wonders at the strangeness of the psychological problem before him; and, in spite of himself, is forced to the conclusion that, practically, the noise that is made at him in the name of humanity is, after all, sounding brass and tinkling cymbal.

BENJAMIN W. RICHARDSON